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THE OLD BEHAVIORISM: A RESPONSE TO WILLIAM BAUM'S REVIEW OF THE NEW BEHAVIORISM

J. E. R. STADDON

DUKE UNIVERSITY

It is hard to know how to respond gracefully to a review as incomplete, inaccurate, and just plain ad-hominem grumpy as Baum's, but I will try. The review is ad hominem because Baum imputes motives—dishonorable ones—to me. It is inaccurate in numerous ways that I shall document. And it is incomplete because it fails to tell the reader what the book is about.

First, Staddon's evil motives: "As I read through his diatribe against Skinner . . . I wondered why Staddon would write this. . . . Most likely, it is a political move; he wants to distance himself from Skinner and to curry favor with the anti-Skinner psychologists and philosophers" (p. 76). A response is hardly necessary, but I remind the reader that smearing the messenger if you can't deal with the message is a standard "political move." As for currying favor with the anti-Skinnerians, perhaps Baum missed the section in *The New Behaviorism* (*TNB*) headed *Philosophical Objections to Cognitive Psychology,* roughly half of chapter 6, which discussed several problems with the cognitive approach, including the homunculus fallacy, the competence-performance distinction (due to the anti-Skinnerian-in-chief, Noam Chomsky), lack of attention to motivation (Guthrie's objection to Tolman), and overinterpretation of straightforward experimental results, illustrated by Schachter and Wagner's (1999) imaginative comments on a brain-recording experiment published in *Science.* Or chapter 8, which refutes several cognitive accounts of phenomena related to consciousness. Or perhaps Baum failed to notice in this very journal the unenthusiastic reaction of cognitivists Gallistel and Gibbon to our critical treatment of scalar timing theory (Gallistel, 1999; Gibbon, 1999; Staddon & Higa, 1999)? Some currying! Some favor!

Baum's review contains so many inaccuracies and omissions that I believe it seriously misrepresents the book. One of the milder is his accusation that Staddon is "promoting himself as wiser or more humane" than Skinner (p. 75). He gives no citation, because there is no such passage in the book. I do question the largely unstated ethical presumptions on which Skinner's prescriptions for society depend, but I do not seek to substitute my own. More serious is his assertion that in a 1995 *Atlantic Monthly* article, I "presented . . . arguments in favor of the death penalty, completely ignoring that *all research so far* [italics added] indicates it is ineffective as a deterrent" (p. 76). This is false and misleading, because the death penalty is not even discussed as a separate issue in that article. What I actually say in the book about legal punishment in general is "I'm not sure whether these . . . speculations on the societal effects of punishment are true or not. Arguments like this are based on a combination of intuition and some laboratory experiments. They can never be conclusive. The behavioral data lend themselves to many often contradictory views" (p. 119). A subsequent debate in the *Wall Street Journal* (Tucker, June 21, 2002, and letters published June 27) shows yet again that the efficacy or otherwise of the death penalty as a deterrent is still a matter for debate. *Some* (not *no*) evidence favors deterrence, but there is always room for doubt. And in my *Atlantic Monthly* article there is nothing that should be construed as advocacy of the death penalty, only a criticism of Skinner's weak arguments against punishment in general—criticisms with which Baum agrees.

Baum's own position on the punishment issue is revealed by his accusation that I go "to ridiculous extremes. [Staddon] includes a long discussion of the necessity and virtues

Address correspondence to John Staddon, Center for Behavioral Neuroscience and Genomics, Department of Psychological and Brain Sciences, GSRB2—Box 91050, Duke University, Durham, North Carolina 27708 (e-mail: staddon@psych.duke.edu).

of punishment in social policy that rests entirely *on the unexamined assumption that punishment works as a deterrent.* [italics added.]'' (p. 76). This statement is both inaccurate and extremely odd. It is inaccurate because I nowhere insist that punishment is a *necessity.* What I do say is that Skinner's arguments against it are scientifically weak. It is odd because Baum seems to be questioning the idea that punishment (often? sometimes?) acts as a deterrent. Does he really think that the habits of drivers will not change if speeding tickets are abolished, or that the prevalence of shoplifting is unaffected by the penalties for it? If Baum really does believe that no one has thought about the issue of deterrence, or that punishment never in fact deters, then I leave it to the reader to decide just who goes ''to ridiculous extremes.''

Baum ''enjoyed [my] criticism of postmodernism'' (p. 76), but asks ''are we supposed to believe that Skinner was responsible for political correctness?'' Well, no, and why on earth would you think so? He is unhappy that I ''blame Smith's excesses on Skinner'' (p. 76). But I was not blaming Skinner, merely pointing out the use that some postmodernists, such as Barbara Herrnstein Smith, have made of his ideas. And in making that connection, I was relying on the writings of postmodernists themselves. Julie Andresen (1991) makes the connection explicitly, and Herrnstein Smith (1986), in her book *Contingencies of Value,* cites Skinner in connection with what I call the Darwinian metaphor and says things like ''literary value is . . . a changing function of multiple variables'' (p. 11)—a turn of phrase that owes more to Skinner than to Derrida.

Skinner rarely, if ever, addressed epistemological issues directly, but when pressed he was probably a philosophical realist and never to my knowledge gave up the idea of truth. Nevertheless, there can be little doubt that some of his confident and all-too-memorable pragmatist pronouncements readily lend themselves to postmodern distortions. At the end of this section, Baum suggests that I am ''just trying to tar Skinner with the postmodernist brush so that [I] can justify including a critique of relativism that would otherwise seem out of place'' (p. 76). Oh, really?

Baum gives an exceedingly misleading impression of the tone of *TNB.* Most readers do not find it ''negative'' or ''a diatribe.'' Some find the writing light, even amusing. Respected science writer John Horgan, a pretty neutral observer (albeit an ''outsider''—see below), called it ''a brilliant, witty critique. . . .'' But the arguments in the book are made as simply and directly as possible, and I have learned the hard way that directness is upsetting to some academics.

Baum writes that the first edition (1993) of *TNB,* was ''a shorter and much less anti-Skinner book.'' It was indeed much shorter, but it was *not* less critical of Skinner, it was more critical. But neither book is ''anti-Skinner'' in the ad-hominem way that Baum implies. Both editions contain a long and laudatory account of Skinner's landmark ''A Case History . . .'' paper, for example (see the section entitled *Skinner's Method* beginning on p. 28 in *TNB*). Indeed, I was concerned in *TNB* to give a fuller picture of Skinner's enormous influence and therefore added several sections including a summary evaluation (pp. 120–123) that acknowledges at some length his contributions as well as what I see as his failings. By any standard, the verdict is of a kind that most of us would be happy to see pronounced on our own life's work.

Some of Baum's comments are simply mysterious. Staddon, he says, ''writes as an outsider, referring to behaviorists as 'they' and disparaging their institutions. Their journals, he reports, publish research 'in the Skinnerian tradition' '' (p. 77). This strikes me as a strange view of science. ''Insider'' and ''outsider'' is the language of tribes, fraternities and ideologies, not of science. The book is addressed to a broad audience—nonbehaviorists as well as behaviorists—so how else should I refer to behaviorists but ''they''? And why is the fact that journals like *JEAB* publish research in the Skinnerian tradition ''disparaging''? Baum's answer: ''The implication appears to be that behavior analysts . . . have made no conceptual advances beyond Skinner's framework'' (p. 77). Have physics journals that publish ''in the Newtonian tradition'' failed to advance? What nonsense! This is not criticism, but paranoia! Ditto Baum's equating the term ''industry'' to ''plodding and directionless'' in the context of research on choice (p. 77). (See Staddon & Cerutti, 2003, for a summary of my real views on choice research.)

The kernel of my difference with Baum about Skinner's contribution seems to be in this passage: ''Staddon calls him a 'brilliant experimenter' and mentions that 'Skinner provided a conceptual framework for understanding learning that (I believe) has yet to be fully explored,' although he can't help adding 'even though his strictures against theory prevented him from exploiting it himself and impeded the efforts of others to do so' (Staddon, 2001a, p. 122). This is damning with faint praise'' (p. 76). (I would call this ''qualified praise'' rather than ''faint praise,'' incidentally. Faint praise would be something like this: ''If Staddon has seen far (and that is an open question), it is because he stood on the shoulders of giants . . .'' [p. 76].) Baum continues, ''Skinner invented the 'Skinner box,' *but his experiments were much less significant than what he found to say about them.* [italics added]'' (p. 76). As my quotation makes clear, I am not unsympathetic to the italicized passage to the extent that it refers to Skinner's ideas about operant behavior, stimulus control, and his many ingenious suggestions about possible processes involved in reinforcement schedule performance. But I could not disagree more if Baum includes Skinner's extravagant extrapolations from pigeons in Skinner boxes to human beings in society. What Skinner had to say in the arena of public policy was much less ''significant'' than his laboratory science.

Baum himself seems to recognize the inconsistency of his position: ''I found myself objecting [to *TNB*'s 'excoriation' of Skinner], even though I agree with most of the criticisms'' (p. 75). And later, in a particularly sugarcoated passage: ''I found its negative tone offensive, its presentation of the place of theory unclear and ambiguous, and much of the philosophical discussion imprecise and halfbaked. This was true despite my agreement with most of the essential points'' (pp. 77–78). I cannot believe that any fair-minded reader would call *TNB* an ''excoriation'' of Skinner. I have simply tried to take him at his word. When he says that no theory should refer to ''events taking place somewhere else,'' I take him literally and conclude that the statement is wrong in a pretty major way. Yes, I know the historical context, but obvious counterexamples—like Mendelian Genetics and Dalton's laws—have been available for a century or more. If all Skinner meant was to argue against naive neurophysiology and folkpsychology mentalism, he should have said so.

Finally, Baum is flummoxed by the term ''internal state.'' He has a Pavlovian reaction to the word ''internal'' and concludes, ''In [chapters 6 and 7] Staddon makes the transition from state variables to internal states without justification and leaves the reader in confusion as to exactly what he intends about their status'' (p. 77). Well, I anticipated a problem with the idea of internal state and wrote chapter 7 to clear it up. I thought I had succeeded when I read ''That sounds all right; state variables are useful in models'' (p. 75). Yes, that's correct: An internal state is nothing but a state variable (hidden variable is an equivalent term). But then Baum goes on to say, ''Despite his claims to the contrary, Staddon, like the cognitivists, thinks he is studying states and mechanisms in the brain'' (p. 75). This is almost the opposite of the truth, because large chunks of *TNB* argue that the first object of behavioral theory is always *behavior.* For example, ''. . . just look at feeding *behavior,* it *is* possible to come up with a very simple model that can duplicate feeding regulation and the basic patterns of feeding under challenge. . . . The model . . . has very few assumptions and explains quite a lot of facts. But the ingredients of the model are designed to explain the behavioral data, not to match up with known physiology'' (*TNB,* p. 153). The problem for Baum is apparently the term ''internal,'' which means, of course, ''internal to the model'' not, or at least not necessarily, ''internal to the organism.'' I don't know why some people find this idea so confusing, but I had another bash at clarifying it in my book *Adaptive Dynamics* (2001, chapters 1 and 4), to which I refer the interested reader.

But to me the most unfortunate aspect of Baum's long review is its incompleteness. He does not tell the reader (as other reviewers have) what the book tried to do. Baum writes ''[non-Skinnerians] rarely read or understand Skinner's writings'' (p. 76), and I would add ''or the writings of anyone in the behavior-analytic tradition.'' Behavior analysis is an isolated movement and part of that isolation was by Skinner's own design (see the box on *Epistemological Isolating Mechanisms, TNB,* p.

34, and also Staddon, 2002), but it has been maintained by many of his followers. One of *TNB*'s main objectives was to reduce behaviorism's isolation not simply from the rest of psychology but also from other parts of social science and the humanities that Skinner totally ignored while he was prescribing for society. I therefore attempted to place behaviorism, particularly Skinner's version of radical behaviorism, in a psychological context that includes artificial intelligence and cognitive science. I described how behavioristic ideas could shed light on topics of interest to cognitive scientists, such as consciousness, the so-called ''binding problem,'' and several visual illusions. If others follow, perhaps behaviorist writing will no longer be treated as irrelevant by the larger psychological community. In any event, behaviorists surely have some obligation to engage other behavioral scientists rather than ignoring them—and being ignored in turn. And Skinner's forays—no, major expeditions during the last 25 years of his life—into large social issues required that I discuss topics in moral philosophy and ethical theory, political science, legal theory and economics. These discussions are necessarily brief because of constraints on space and my own intellectual limitations. But the attempt to provide some context is, I believe, a major contribution of the book and an essential counterweight to Skinner's solipsistic style and the isolation of behaviorism to which it led.

Seen from outside behavior analysis, Skinner's intellectual arrogance is absolutely incredible. He writes as if no one else had ever had a worthwhile thought about such matters as penal reform, the raising of children, and the organization of education. Even more incredible is the fact that no one—no behaviorist, at any rate—called him on it, so great was the prestige of ''science'' and his (admittedly substantial, but hardly decisive for human affairs) contribution to it. As I point out in the book, many of Skinner's ideas in these areas (like token economies and the systematic application of incentives—''reinforcers'') had been examined or even actually tried much earlier—not always with the happy results Skinner predicts.

Baum begins his review by saying ''This book is about theory.'' But *TNB* is not really ''about theory,'' it is about behaviorism. Three chapters are devoted to a critical evaluation of Skinnerian behaviorism, because that is the only variety alive today as a separate entity. The emphasis of the book is on philosophy because ''Behaviorism is not the science of human behavior; it is the philosophy of that science'' (Skinner, 1976, p. 3). Hence I describe experimental and theoretical work only as needed to understand the philosophy and evaluate the impact of behaviorism. It is not a review of operant conditioning theory. Hence Baum's unhappiness with the relatively infrequent citations of contemporary behaviorists not unrelated to Baum is inappropriate.

And what is *my* philosophy of behaviorism? Baum thinks he knows: ''I welcomed the criticisms of the computer analogy and representations, but noticed that Staddon carefully avoided criticizing the antibehavioral aspect of cognitivism: The denial of behavior as a subject matter in preference to an idea that behavior is only the evidence of inner processes that are the real subject matter. The reason is easy to find: Staddon embraces that very idea. He tells us first that theoretical models incorporate internal states and second that 'These models *are* the behavior . . . what the organism is 'doing,' described in the most colorless, direct way possible' (Staddon, 2001a, p. 144). To me, this statement seems indistinguishable from the cognitivists' competence-performance distinction'' (p. 77).

Set aside for the moment the fact that I devote a long section in chapter 6 to criticism of the competence-performance distinction. Baum's real point seems to be that I do not accept what he takes to be the fundamental credo of behaviorism, namely that behavior is a subject matter in its own right. The problem, of course, is what do you mean by ''behavior''? If behavior is ''uninterpreted physical movement'' then we cannot distinguish between ''waving'' and ''drowning'' or any of the myriad other cases in which the same physical event has different significance at different times. So there are two choices: Either define behavior in increasingly abstract historical ways so as to accommodate its real complexities, or stick close to something like the physical definition and permit proliferation of state variables. Baum favors the former, whereas I embrace both but tend to favor the latter. But I believe we both accept,

with slight differences, the other aspects of behaviorism: that phenomenology has no special status, that explanations for behavior need not (Staddon) or should not (Baum and Skinner) refer to physiological variables, and so on. Henry Kissinger says somewhere that academic controversies are so bitter because the issues are so trivial. Score one for Henry!

To paraphrase Baum, as I read through his diatribe, I wondered why he would write this. Unlike Baum, I have no answer, but a colleague who read his review commented, ''You can't afford to be an iconoclast without upsetting the priests!'' I thought behavior analysis was science, not religion, but maybe I was wrong.

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